

CALIFORNIA INSTITUTE OF TECHNOLOGY
PASADENA, CALIFORNIA

DIVISION OF BIOLOGY

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Dear Josh,

I had forgotten that Rowley tested our temperature mutants. I did keep them around for a couple of years, thinking somebody might want to use them, but they finally got thrown out. Sorry I can't help you--you should have written 3 years ago!

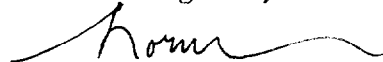
Are you, by any chance, getting one of the free trips to Japan? If so, maybe we could travel part of the way together and have a seminar on gene-enzyme problems.

I don't think you are quite right in saying that the older Neurospora papers contain wild statements about genes making enzymes. In glancing through the papers you cited, I was pleased to find that they are carefully worded and say no more than was intended. You find phrases such as "genes control enzyme production", or "determine enzyme specificity" or "act somehow as a template", which may be vague, but certainly not idiotic. I think that people have read into these statements more than was intended. Perhaps we should have been explicit in spelling out what was not meant, but one likes to assume a common semantic background when addressing fellow scientists, and this may not be justified. I criticize the older papers for not having made clear that when enzyme specificity was being talked about it was not just substrate specificity, as some people now seem to think, but all aspects of enzyme specificity. I also criticize them for having placed the "one enzyme per gene" and the "one gene per enzyme" hypotheses on an equal footing.

With regard to your Mal-Lp example, if you will tell me what the chemical basis of virus resistance is, I will undertake to explain the pleiotropism. For all I know, resistance to lambda may be as complicated as the ability to compose a sonata. Your case is no more surprising than the threonine-methionine pleiotropism was when we first discovered it; they only become simple when you work them out.

Anyway, I am glad that we agree as much as we do on these matters. Personally, I believe that, considered as a purely empirical finding, the one-to-one relation is the most important thing that has been found out about mutations in the last 20 years. It seems odd that other intelligent people don't always agree with me about this.

Best regards,



Norman Horowitz

ADVANCES IN MICROBIAL PHILOLOGY

N. H. Horowitz

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I was startled to read in the September 5, 1952 issue of Science, under the heading "Reactions of the Porpoise to Ultrasonic Frequencies" by W. N. Kellogg and R. Kohler, the following: "Projection of these frequencies into the water was accomplished by a USRL transducer...." On looking up transducer in Webster's New International Dictionary I found this definition: "A device activated by power from one system and supplying power in the same or any other form to a second system. For example, a telephone receiver...."

Out of idle curiosity I thumbed through the dictionary (a 1936 edition) looking for others of the new words of microbial genetics. I found two more; namely, prototrophic and syntrophic. The latter is defined in a way which is consonant with its current usage in microbial genetics and need concern us no further. Prototrophic, however, is defined as follows: "Deriving nutriment, or the energy of anabolism, from uncombined elements, as the nitrogen-fixing bacteria and sulfur bacteria." There is no sign that this usage is obsolete. In fact, it can be found in recent bacteriology textbooks, such as Tanner & Tanner, and Frobisher. (Frobisher incorrectly makes it synonymous with autotrophic).

These discoveries undoubtedly prove the utility of articles on porpoise physiology, even for microbial geneticists. In addition, they raise questions concerning the status of the words transducer and prototrophic. On the principle that one should never court confusion when it can be easily avoided, it would seem to follow that these words should no longer be used with the denotations they have recently acquired in microbial genetics.

These findings also bring up the question of how much philological research may be expected of an author who desires to launch a neologism, and beyond this

the more fundamental problem of deciding in each instance whether or not the coining of a neologism is justified. In connection with the latter problem I should like to suggest that one must avoid the temptation to fabricate neologisms merely as a matter of convenience. Convenience by itself does not provide sufficient justification for a neologism. There must also be a need for the new term. Otherwise we shall soon have a jargon which is intelligible only to a priestly class of initiates and which hinders rather than facilitates communication.